connected facts which relate to them. The student will, in this section of the work, find full directions for performing most of the experiments, which will, when all repeated, enable him to advance on a thorough and sound foundation. Great care is taken to render evident the phenomena of electrotonus, and the subject of tetanus is dwelt on in detail, the following being the propositions which are discussed and proved regarding it:—1. "Tetanus from an ordinary interrupted current is a continuous contraction rapidly reaching a maximum, continuing (within limits) in that condition so long as the current is passing, and followed by a gradual relaxation upon the current being cut off." 2. "Tetanus really consists of a series of simple muscular contractions fused together." The apparatus necessary for verifying these and other points in which electricity plays a part, is described as far as is necessary for the wants of the physiological student, and some, as Wippe's double key and Du Bois Reymond's rheocord, are figured. Several of the points insisted on appear to be insignificant in themselves, but they must all, in the long run, have important bearings on future theory.

In undertaking the "Physiological Chemistry," Dr. Brunton has had a somewhat easier task than the two authors last referred to, and his work is excellent. The results of Hoppe-Seyler, and other German chemists, which are as convincing as they are connected, are fully entered into, and the chemistry of digestion and excretion, together with the method of arriving at them, are explained at considerable length. As an instance of the manner in which the subject is handled, the following are the propositions which are demonstrated in connection with the fact that pepsin is not destroyed during digestion. 1. "Although the digestive power of pepsin appears to be indefinite, yet a limited quantity of gastric juice will not dissolve an unlimited quantity of fibrin." 2. "The arrest of digestion in this experiment (the proof of the previous proposition) is not due to the destruction of pepsin, but to the accumulation of the products of digestion in the liquid and to the want of acid." 3. "A stronger acid is required for digestion if the products of digestion are present in quantity in the solution." The theory of digestion, together with the action of the vagus and splanchnics on the stomach are fully discussed, and the unassuming way in which the author states his own opinions carries great weight with it.

We should have liked to have seen a separate chapter on the methods to be used for rendering animals insensible, together with a notice of the relative value of different anæsthetics and the way to exhibit them; as it is, the subject is only incidentally mentioned in connection with special operations. If the drawings of the instruments had been incorporated in the text they would have been more easily referred to, and therefore more frequently looked at; as it is, the one volume without the other is difficult to understand. The anatomical sketches, mostly after Bernard, which illustrate the distribution and relations of the nerves and vessels that so frequently have to be manipulated by operating students, adds much to the completeness of the work, in which every effort has evidently been made to put the student in as good a position with regard to the subject as can be desired.

The three accompanying woodcuts are from the second volume of this work. The largest is an example of the size and character of the excellent illustrations in Dr. Klein's histological section. Dr. Sanderson contributes that illustrating the relations of the pneumogastric nerve in the frog, and the third is one of the several electrical instruments described by Dr. Foster.

WILSON'S INORGANIC CHEMISTRY

Inorganic Chemistry. By the late George Wilson. M.D., F.R.S.E. Revised and enlarged by H. G. Madan, M.A. (London and Edinburgh: W. and R. Chambers.)

LIKE so many of our old friends among the best books on chemistry, the present edition of the late Prof. Wilson's Inorganic Chemistry has undergone somewhat extensive alterations, and received considerable additions, which, in the opinion of its able editor, have been rendered necessary by the recent progress of chemistry. The original plan, which is that adopted in some of our best text-books, has been adhered to, viz. of introducing the student to a knowledge of the more important fundamental laws of chemistry, and to make him familiar with the properties of the chief elementary substances, and their more remarkable compounds. What is generally known by the name of chemical physics occupies about one-fourth of the whole book. This portion is clear and concise, and deserves the highest eulogium. It may be perused with advantage by every chemical student. The theory of atomicity of elements, which is fast giving a new impression to organic chemistry, and which by some of our most eminent chemical teachers has of late been introduced into the domain of inorganic chemistry also, and which promises to reconcile and harmonise both branches of chemical science, has received but scanty recognition at the hands of the editor, although he professes to have brought the chemical nomenclature (in deference to the wishes of the publisher), into accordance with the system adopted by Profs. Frankland and Williamson.

Professor Wilson seems to have felt that physical and chemical laws cannot be studied with advantage without having some physical and chemical facts to work upon, and the pupil is therefore recommended to read the first 108 pages, treating of chemical physics, with some care before proceeding further; but "he is not to expect to understand the introductory portion at once, but must go back from time to time to their study, when he will find them more and more intelligible as he grows familiar with the properties of chemical substances," explained in the later pages. Is not this an admission that the plan upon which the book is constructed is a faulty one? Is it not time to relegate chemical physics to physics proper, especially when we have such excellent elementary text-books as Balfour Stewart's and others, and to treat of chemical changes in chemical text-books? Not that we would have it inferred that chemical changes can be understood without a knowledge of the general properties of matter, of heat, light, and electricity. By far the greater number of chemical changes being dependent upon chemical affinity, the laws of chemical combining proportions and volume composition can very well be explained by confining the teaching at first mainly to chemical changes. Physical considerations, especially at

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a time when they undergo such rapid extensions, should form the crowning part of chemical studies, and the interdependence of the two branches of science can only be established upon a sound basis when a thorough knowledge of either science has been acquired.

The main portion of the book comprising the chemistry of the non-metallic and metallic elements, arranged under the usual headings Preparation and Properties of the different elements and their compounds, contains much that will highly recommend itself. The more important compounds are dealt with in a manner which will help the student over most of the difficulties he encounters at first, and will enable him to lay a good foundation for more extensive chemical studies. The classification of the metals according to their atomicity—open to objections as it stands—is not always reconcilable with the analytical summaries or tests given after each group of metals, nor are the analytical explanations always accurate. On p. 393, *e.g.* we notice: "Calcic sulphate cannot produce a precipitate in a salt of calcium, because there is more than enough of water present to retain dissolved all the sulphate that can possibly be formed."

There can be little doubt that the new edition of Wilson's Chemistry will be welcomed by all who desire to get a general insight into the science, and that it may be studied with advantage in preference to many larger and more ambitious text-books.

OUR BOOK SHELF

Verhandlungen der k. k. geologischen Reichsanstalt. Nos. 11 to 18. (Vienna.)

THESE numbers of the Proceedings of the Geological Society contain many useful papers, chiefly, however, of local interest. Felix Karrer notes the occurrence of mammoth remains at Vienna. They were obtained during the sinking of a well in a "diluvial" (glacial) deposit at a depth of 9 fath. 3 ft. from the surface. Dr. Lenz also has a short reference to a similar discovery of the teeth of a young mammoth in a brown laminated loam near Nowakmühle. Dr. Stur gives an interesting account of his own and Baron Petrino's observations on the superficial deposits in the basin of the Dniester in Galicia and Bukowina. A great accumulation of loess covers a wide area in that district, the land shells and mammalian remains in which enable these geologists to correlate it with the loess of the Rhine and other regions. We observe, in No. 11, an important table showing Dr. C. E. Weis's systematic arrangement of the carboniferous formation and the rothliegendes formation of the Saar-Rhine district, which is well worth the attention of English geologists. The usual admirable literary notices, and other miscellaneous matter, are appended to each number of the Proceedings.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Leaf-Arrangement

THE chief part of the Rev. G. Henslow's objections (NATURE, vol. vii., p. 403) to my condensation-theory of leaf-arrangement are due to a double oversight on his part. First, he has overlooked the condition of *contact* among the balls which I use to represent embryo leaves. Second, he has overlooked the fundamental position, that leaf-order exists for, and is determined in, the *bud.*

The bud requires economy of space. This involves contact among the embryo leaves; and if we experiment with balls attached (as described in my paper) in two rows alternately on either side of a contractile axis, we shall see that when the axis is allowed to contract with a twist, that twist is necessarily limited by the conditions of contact which arise, and that we cannot "cause it to make a complete rotation if we choose." Given the size of the balls and their distance from the axis, the position which they will assume (under contraction with a twist) is necessarily determined by the geometrical conditions of mutual contact. This consideration furnishes the answer to Mr. Henslow's italicised query, and also to his two previous questions (1) and (2). It also gives back a "really explanatory meaning" to my expression "*maxima* of stability," for if we have one sphere standing almost vertically on another and supported by a third and a fourth to right and left, we have therein some statical conditions which admit of maxima and minima of stability. The same consideration also removes the objection that "the positions taken up by the balls must be arbitrary, or at least in proportion to the twist given by the hand—a perfectly arbitrary force." The twist given by the hand in my experiment serves only to determine the direction of the real twist; the subsequent real availing twist is insisted upon by the two ranks of balls-in-contact as the sole condition of obedience to the contractile force of the indiarubber axis; and this twist is limited by the conditions of contact above described. Let the direction of the twist be given, and there is nothing arbitrary in the result.

The objection that "if an axis becomes twisted the *fibres* will be twisted also" loses force if we bear in mind that the leaf-order is imposed upon the embryo leaves in the very earliest stage of their bud-life; and that the formation of fibres, taking place at a subsequent stage, must find itself compromised by an already existing arrangement of the embryo leaves. The elastic band in my experiment, "if it were a pliant shoot," would certainly "contort the vessels and wood-fibres;" but it was not meant to represent a "pliant shoot" except in its earliest embryonic bud-stage, and that at some very remote period in the past.

I must ask Mr. Henslow to bear in mind that he has before him only the abstract of my paper, and that necessary brevity has left some points too bare, and has wholly suppressed others of small importance. Among the latter is some mention of the "secondary series" $\frac{1}{3}$, $\frac{1}{4}$, $\frac{2}{7}$, &c., which, though it may be found in the abnormal variations exhibited by a cultivated plant like the Jerusalem Artichoke, yet cannot be reckoned with examples of normal leaf-order.

Let me take this opportunity of insisting again on the astonishing agreement between the facts of nature and the results which the condensation-theory leads us to expect. Taking one member to start from as 0, we find in nature that the members in contact with 0 belong to the following series, 1, 2, 3, 5, 8, 13, 21, 34, 55, 89, 144, &c., and these are the very same members which would necessarily be brought into contact with 0 under successive degrees of condensation with twist from an original order $\frac{1}{2}$.

I have lately met with a striking confirmation of the truth of the condensation-theory. The simplest order of the whorled type is that in which the leaves stand in pairs, decussate. Now if we consider what would be the result of condensation with twist applied to this arrangement, we can see that it would produce a new series of orders, in which the following members would successively come into contact with 0,—2, 4, 6, 10, 16, 26, 42, &c., and would present the phenomenon of 2, 4, 6, 10, &c., spirals alternately to right and left. This result is exemplified in nature. The teazle (*Dipsacus silvestris*) has the decussate order in its leaves; and in its head (where we might expect to find its leaf-order condensed) we count sixteen conspicuous spirals in one direction and twenty-six in the other:—that is to say, we have 0 in contact with No. 16 on one side and No. 26 on the other. No. 42 stands higher between 16 and 26, but inclined towards the former. No. 10 stands next below 26, and No. 6 next below 16. These numbers belong to the new series above mentioned.

This close parallel between fact and theory appears to me to give a value to the latter which it will not lightly lose.

March 30 HUBERT AIRY

The Hegelian Calculus

AS Dr. J. H. Stirling has enjoyed the exceptional privilege of replying contemporaneously to my paper on Hegel in the

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